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it is a matter of indifference whether they be generally accepted at a late or early season, in my own lifetime or not till long afterwards.

The Rotation-Period of Mars. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

In the *Annalen der Sternwarte in Leiden*, for 1872, there is a series of papers on the planet *Mars* by the late Prof. Kaiser. One of the papers relates to the rotation-period of the planet, and Prof. Kaiser therein endeavours to show that his estimate of the rotation-period of *Mars* — viz. $24^h\ 37^m\ 22^s{\cdot}62$ — is preferable to mine — viz. $24^h\ 37^m\ 22^s{\cdot}735$. When I first read this paper, it appeared to me that, whether Kaiser were mistaken or not in his conclusions, a great difficulty was introduced; because he stated as the result of his calculations, that a drawing of *Mars* by Huyghens, in 1672, cannot be reconciled with the two drawings by Hooke, in 1666, on which I placed reliance, unless a mistake of two hours in the time be accounted for. Even then,— that is, after my first cursory reading of his paper,—it seemed to me that the readiest solution of the difficulty was not, as Kaiser suggested, to reject the work of one or other observer, but to divide the error between them. Yet it seemed clear to me that this would involve the supposition of very careless drawing *both* by Huyghens and by Hooke. I have been glad, however, to find, when my leisure permitted me thoroughly to examine Kaiser's work, that the greater part of this discrepancy is accounted for by a rather strange error in his reckoning. In estimating intervals between the observations in the seventeenth and nineteenth centuries he would seem to have forgotten that the years 1700 and 1800 were not leap years, and so makes the intervals systematically two days too great. As he thus gets two Martian rotations too many, and each exceeds a terrestrial day by $37^m\ 22^s{\cdot}7$, the error amounts approximately to the loss of $1^h\ 14^m\ 45^s{\cdot}4$. This reduces the discrepancy by more than one-half. Oddly enough, a second error in calendar matters has prevented Kaiser from detecting the error, as he might have been expected to do when he compared the observations of Hooke and Huyghens. For Hooke's observation, made on March 3, Old Style, he translates into March 14, New Style, whereas the real reading is March 13. Thus an error of $37\frac{1}{2}$ minutes is introduced as between Hooke's observations and Huyghens', and this raises the discrepancy between them so nearly to that which *should* exist if Kaiser's rotation-periods were correct, as to satisfy the requirements of the case.

As regards the balance of difference between the two esti-

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mates, I believe what I shall now proceed to say can scarcely fail to be regarded as a fair view of the evidence on both sides; and inasmuch as it gives (for the first time) an account of the whole evidence, so far as the most ancient observations of *Mars* are concerned, it must be held as for the present determining the most probable value of the planet's rotation-period.

Let it first be noted that the two estimates lie very near each other. A difference of $0^{s}\cdot115$ or $\frac{23}{200}$ths of a second, cannot be regarded as serious; and when correction is made for Kaiser's error as to the years 1700 and 1800, the difference is greatly reduced. And here I may remark that I fully agree with Kaiser's opinion, that Linsser and others exaggerated the accuracy with which the rotation-period can be determined. I note that, although I give the seconds to the third decimal place, I only do this to indicate the most probable value. I mention that the value appears to me to lie between $22^{s}\cdot73$ and $22^{s}\cdot74$. Kaiser sets the value at $22^{s}\cdot622$ with about equal confidence in the two last decimal figures. It seems to me to follow from the evidence cited in his latest paper, that he and I have ourselves erred slightly in the point we have both dwelt on as involving a mistake (though a larger one) on the part of others, and that at present we must be content to regard the probable error as somewhat larger than the value at which we had agreed in estimating it.

The question turns on the ancient observations employed; for, as Kaiser very justly remarks, when an old observation is compared with modern observations taken within a comparatively short interval of time, the resulting rotation-periods will be almost identical, even though the two modern observations be affected with a considerable error. I must confess, however, I cannot understand why this remark is expressed as a correction on my own statements respecting the two observations in 1864 and 1867, which I compared with Hooke's observations in 1666; seeing that I prefaced those remarks with a statement to precisely the same effect as Kaiser's ostensible correction.

The (corrected) difference between Kaiser's result and mine arises from our selection of different observations in the seventeenth century to start from. Kaiser depends on a drawing by Huyghens, fig. 1, bearing date, August 13, 1672 (New Style) at half-past ten, Paris time. I employed two drawings by Hooke, figs. 2 and 3, taken on March 12, 1666, at $12^{h}\,20^{m}$ and $12^{h}\,50^{m}$ Greenwich time (New Style, and astronomical hour).

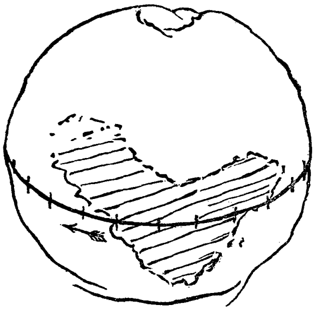

Fig. 1.

Taking Hooke's pictures for comparison with observations made in 1864, 1867, and 1869, I obtained the rotation-period $24^{h}\,37^{m}\,22^{s}\cdot735$. Singularly enough, Kaiser, using his own observations in 1862, for comparison with Hooke's, obtains precisely the same period, even to the last decimal place, though

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there is a triple discordance between his treatment of the matter and mine, for he sets the date of Hooke's observation one day too late, counts two days over by taking 1700 and 1800 as leap-years (I assume this is the way in which he gets his two extra days), and using his own observations in 1862, introduces a considerable time error, unless Beer and Mädler, Dawes, Browning, and others, have all made large time errors. Using an observation by Beer and Mädler, in 1830, he obtains, with Hooke's, the period $24^{h}\ 37^{m}\ 22^{s}{\cdot}706$, or $\cdot 029^{s}$ less. He dwells on this as a strong point against my estimate, failing to notice that, in his own treatment of his own estimate, he deduces from the observations of 1862, combined with Huyghens', the period $24^{h}\ 37^{m}\ 22^{s}{\cdot}643$, while he deduces from the observations of 1830, combined with Huyghens', the period $24^{h}\ 37^{m}\ 22^{s}{\cdot}595$, a difference of $\cdot 048^{s}$. This, of course, was to be expected, from the singular medley of obvious errors appearing in a paper laboriously exact in more recondite matters.

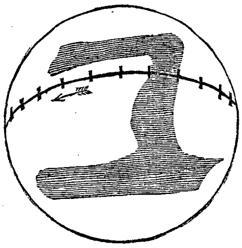

Fig. 2.

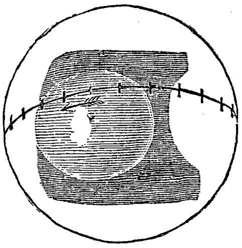

Fig. 3.

Kaiser deduces from Huyghens' observations, combined with a great number of modern observations, the value $24^{h}\ 37^{m}\ 22^{s}{\cdot}622$. In discussing Hooke's observations he introduces only one day in excess, because of the mistake he made in correcting Hooke's date for style. Huyghens' observations being dated in new style, there is no compensation of errors, and thus we must take into account the two extra days derived from the common years 1700 and 1800, treated by Kaiser as leap-years. Kaiser makes the interval between Nov. 1, 1862, $6^{h}\,10^{m}{\cdot}1$, and August 13, 1672, $12^{h}\ 10^{m}{\cdot}3$, (at which epochs he finds that *Mars* was in the same position, as regards sidereal rotation), to be $69476^{d}\ 17^{h}\ 59^{m}{\cdot}8$; and he proceeds, "In this period *Mars* made 67719 rotations; and the resulting rotation is $24^{h}\ 37^{m}\ 22^{s}{\cdot}643$." In reality the interval is $69474^{d}\ 17^{h}\ 59^{m}{\cdot}8$, and in this interval *Mars* made 67717 rotations; hence the calculated rotation-period is $24^{h}\ 37^{m}\ 22^{s}{\cdot}71$; very near, it will be observed, to my estimate. However, as will be inferred from a former remark of mine, Kaiser's observations in 1862 do not accord with those made by several other observers. He himself notes this; and if we adopt his mean from various modern observations, we infer, after correction for the extra two days, the period $24^{h}\ 37^{m}\ 22^{s}{\cdot}69$, by combining the observations of Huyghens with those made in modern times.

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Kaiser was not aware of the existence of Hooke's observations when he used Huyghens'; and though I was aware of the existence of Huyghens' drawings (for Webb mentions them in his *Celestial Cycle*), I had not seen them when I used Hooke's.

Commenting on Kaiser's value, I remarked that it would throw the planet out at the period of Hooke's observations by an amount corresponding to the rotation during $2^h\ 20^m$. This would correspond to a rotation angle of about 34°, and would set the features shown in figs. 2 and 3, towards the *right*, making the north-and-south sea fall very near the edge. "This," I wrote, "is not to be thought of for a moment."

On the other hand, it appeared to Kaiser, in consequence of the mistakes already noted, that my period would throw the planet out at the period of Huyghens' observations by about the same amount, the great sea shown there being thrown by about 32° rotation towards the *left*. Kaiser reasons that *this* cannot be thought of; and that, therefore, choice must be made between Hooke's pictures and the drawing made by Huyghens. He conceives that there can be no question that the sea in Huyghens' picture is the sea which, for convenience of reference, I have called Dawes' Ocean; and that it is very doubtful whether Hooke's picture represents the same Ocean with Kaiser's Sea running north and south.* On the first point I fully agree with him. The feature depicted by Huyghens can be no other than the Dawes' Ocean; indeed, even if the supposed difference of two hours were assumed to operate, the Dawes' Ocean would still occupy the disk of this planet estimated for the hour of Huyghens' observations. But I can by no means agree with Kaiser that the feature depicted by Hooke is doubtful. It does not seem to have occurred to Kaiser to construct a picture of *Mars*, at the time of the observation in 1666, *using his own rotation-period.* If he had done this, he would have seen that the continent called Herschel I. would fall over the space where Hooke shows a well-marked north-and-south sea. Absolutely no feature on *Mars*, except the Kaiser Sea, ***could*** look like the north-and-south sea depicted by Hooke; but the region which would fall where this sea is shown, if Kaiser's period is correct, is the most unlike conceivable.

In fact, I think Kaiser would not have remarked on the uncertainty of Hooke's drawing if he had seen views of this planet at the same rotation-phase, and when in the part of its orbit where Hooke observed it. Hooke's aspect is quite as characteristic for the summer season of the Martial northern hemisphere as Huyghens' for the winter season of the same hemisphere. As

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* "Hieraus würde folgen," he says, after stating the facts, "dass die Abbildungen von Hooke und Huyghens nicht denselben Fleck darstellen können. Es scheint mir nicht schwer zwischen beiden eine Wahl zu treffen. Huyghens's Darstellung ist eben so bestimmt und sicher als die von Hooke unbestimmt und unsicher ist. Hat Huyghens den Flecken f. (Dawes Ocean) wirklich gezeichnet und sich in der Zeitengabe nicht gänzlich geirrt, so muss Hooke's Abbildungen für eine Bestimmung der Rotationszeit unbrauchbar sein."

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to the accuracy of the two drawings there is not much to choose perhaps. One cannot be enthusiastic over the artistic qualities of Huyghens' drawing, and the fact mentioned by Kaiser, that of twelve drawings by Huyghens, this is the only one in which any known feature of *Mars* can be recognised,* does not suggest either excellence of telescopic performance or surpassing skill in delineation. Hooke's drawings are in some respects much better; but it is easy to perceive from the aspect of other drawings by Hooke representing *Mars* as his distance increased, that the telescope employed was barely equal to show even the Kaiser Sea when *Mars* was close to opposition. This would introduce a difficulty in the delineation of the planet which telescopists and draftsmen will be familiar with. Accurate drawing becomes very difficult, even to skilful artists, when the features to be delineated are only to be seen at intervals, and then only (when definition is for the moment at its best) with exceeding faintness.

It seems to me that unless we absolutely reject the work either of Hooke or Huyghens, we must use both. If I had known of Huyghens' observations, I should certainly not have trusted implicitly to Hooke's; and on the other hand, I cannot agree with Kaiser in trusting implicitly to Huyghens'. It is strange that the idea should not have suggested itself to Kaiser, that though the whole difference of upwards of two hours' rotation cannot possibly be ascribed either to Huyghens' work or to Hooke's, it is perfectly possible to divide the error between Hooke and Huyghens.

Let us first consider to whose work the greater proportion of the error should be ascribed. I think we cannot prefer Hooke's work to Huyghens'; first, because Hooke gives two drawings closely according, and secondly, because the only part of Huyghens' drawing which includes the real outline of Martian oceans lies near the edge of the disk. The southern boundary of the Dawes' Ocean, as seen, is known to be variable, owing to clouds or other peculiarities of the Martian atmosphere. It is also always ill-defined; but the northern boundary is well marked. Now any feature near the edge shifts very slowly owing to rotation; and although two hours would certainly change the aspect shown in Huyghens' picture to one differing too much from it to be mistaken, yet one hour's rotation could fairly be ascribed to errors in his very rough drawing. On the other hand, it is not easy to suppose that *both* Hooke's drawings would be in error by so much as three-quarters of an hour's rotation, simply because a feature like the north-and-south sea changes in apparent position when approaching the centre of the disk more notably by far than a curved outline of an ocean like that shown in Huyghens' picture.

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* Kaiser writes:—"Unter den 12 Abbildungen des Planeten Mars, welche Huygens in seinem Tagebuche mit der Schreibfeder eingetragen hat, giebt er nur eine einzige, in welcher sich einer der in den Jahren 1830, 1862, und 1864, beobachteten Flecken mit voller Gewissheit zurückfinden lässt."

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Fortunately, when Kaiser's calendar mistakes have been taken into account, we have no occasion to attribute such large errors either to Huyghens or Hooke. I will give Kaiser's comparison between the two observations, and then the just comparison, so that not only the actual correction may be indicated, but the proportion which it bears to the correction Kaiser supposed to be necessary.

Regarding Hooke's observations as I have done (but with the object of showing I was mistaken), he finds the following epochs for the passage of the Kaiser Sea through the same part of a sidereal rotation :—

		h	m
Huyghens	1672, August 13,	12	10·6
Hooke	1666, March 14,	2	56·1
Difference	2344 days,	9	14·5

In the interval *Mars* made an exact number of rotations ; *but* taking the period as $24^h\ 37^m\ 22^s{\cdot}62$, we find—

2285 rotations = 2344 days $7^h\ 26^m\ 27^s$.

a difference of nearly two hours. This would indicate, of course, a discrepancy of two hours between the indicated rotation-phases.

The just comparison is as follows :—

		h	m
Huyghens	1672, August 13,	12	10·6
Hooke	1666, March 13,	2	56·1
Difference	2345 days	9	14·5

In the interval *Mars* made an exact number of rotations ; *but*, taking the period as $24^h\ 37^m\ 22^s{\cdot}735$, we find

2286 rotations = 2345 days $8^h\ 8^m\ 12^s$.

a difference of $1^h\ 6^m\ 18^s$.

This discrepancy is easily disposed of. Note first, that when the discrepancy is thus reduced, the supposition that Hooke depicted some other feature than the Kaiser Sea must at once be dismissed. Then, an error corresponding to 40 minutes' rotation may be readily assigned to Huyghens' drawing, and one corresponding to rather more than 26 minutes' rotation to Hooke's. This seems a fair apportionment of the discrepancy, and a very brief inspection of the figures will show that the difference in the drawings would be insignificant.

Now if we assign 26½ minutes of the error to Hooke's drawing, setting the Kaiser Sea towards the right by the rotation in

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that interval, we get for the rotation period $24^h\ 37^m\ 22^s{\cdot}713$, instead of my former estimate, $24^h\ 37^m\ 22^s{\cdot}735$. And this seems the result according best with the evidence; or one may say that the rotation period is fairly represented by the value, $24^h\ 37^m\ 22^s{\cdot}71$.

It may be noted that if we ascribe the whole error to Hooke, we get the period $24^h\ 37^m\ 22^s{\cdot}681$, whereas if we ascribe the whole error to Huyghens, we get the period $24^h\ 37^m\ 22^s{\cdot}735$, and the period may be regarded as certainly lying between these extreme values (only differing by $0{\cdot}054^s$).

I conceive that had Kaiser detected the calendar errors in his computation, he would not have contended for the rejection of Hooke's observations. By applying to the ancient observations his method of taking the probable mean of modern observations, we deduce the rotation period $24^h\ 37^m\ 22^s{\cdot}71$, or perhaps giving due weight to the fact that Hooke made two drawings, we may infer a value lying even nearer to my former estimate. When we note the roughness of Huyghens' drawing, and the unsuitability of the aspect he has pictured for deducing a time-estimate, we may not unfairly consider that a somewhat larger proportion of the error should be awarded to Huyghens. Hence we should deduce some such period as $24^h\ 37^m\ 22^s{\cdot}72$. But the difference between this result and the other is not worth special discussion. The truth probably lies between these values,—

$$\begin{array}{lccc} & 24^h & 37^m & 22^s{\cdot}71 \\ \text{and} & 24 & 37 & 22\ {\cdot}72 \end{array}$$

The Distribution of the Clusters and Nebulæ.

By Sidney Waters, Esq

The charts which I now beg to lay before the Society contain all the objects recorded in Sir John Herschel's general catalogue of 1864.

The projection is isographic, equal areas in the heavens being represented by equal areas *in plano*, and the objects are marked within one degree in P. D., and 4 minutes in R. A. of their true places.

It will be seen that the contents of the Catalogue have been divided into three classes:—1st. Clusters (globular and irregular); 2nd. Resolvable Nebulæ; 3rd. Irresolvable Nebulæ; each class being distinguished in the maps by a distinctive mark.

A careful study of Mr. Proctor's Distribution Charts, published in the *Monthly Notices* in 1869, convinced me that by applying this method of charting, a further insight would be gained into the laws of nebular distribution, and I believe that